

Collective flow and QCD phase transition

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In the first part I discuss the sensitivity of collective matter expansion in ultrarelativistic heavy-ion collisions to the transition between quark and hadronic matter (physics of the softest point of the Equation of State). A kink in the centrality dependence of elliptic flow has been suggested as a signature for the phase transition in hot QCD matter. Indeed, preliminary data of NA49 presented at this conference show first indications for the predicted kink. In the second part I have a look at the present theories of heavy-ion reactions. These remarks may also be seen as a critical comment to B. Mueller's summary talk (nucl-th/9906029) presented at this conference.

1. A Kink in the Centrality Dependence of Elliptic Flow

Information about the quark-gluon plasma (QGP) and the phase transition region has become available with the advent of powerful lattice simulations of quantum chromodynamics (QCD). Most notably, it has been shown that chiral symmetry is restored at rather low temperatures (in the range 140 to 170 MeV). Furthermore, the Equation of State (EOS) varies rather rapidly in the transition region. It is not clear yet whether the transition is of weak first-order or just a rapid cross-over between the two phases. The EOS extracted from the lattice clearly displays the transition from hadron to quark-gluon degrees of freedoms. Pressure and energy density approach the ideal Stefan-Boltzmann values at temperatures $\geq 3T_c$. A generic feature of the EOS in the transition region is the presence of the so-called "softest point of the EOS" [1,2] related to the effect that the energy density may jump with increasing temperature but the pressure does not.

The collective transverse flow which develops in the heavy-ion collisions reflects on the properties of the EOS. Usually, one distinguishes various types of transverse flow, the radial (isotropic component), directed (sideward kick in the reaction plane) and the elliptic flow, the latter being a preferential emission either along the impact parameter axis or out of the reaction plane (squeeze-out) [3]. The general idea why a phase transition may show up in flow observables is rather straightforward. At densities around the softest point the force driving the matter expansion gets weakened. A long time ago, van Hove has suggested that the multiplicity dependence of average transverse momenta may display a plateau and a second rise [4] which was not seen, however. Some time ago I have suggested that the elliptic flow may be better-suited to identify a first-order type phase transition [5].

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Elliptic flow in the central region of ultrarelativistic collisions is driven by the almond-shape of the participant matter in the transverse plane [6]. It was argued in [5] that elliptic flow may be more sensitive to the early pressure than the isotropic radial flow. “Early” and “late” is defined by the time scale set by the initial transverse size $r_t = \sqrt{\langle x^2 + y^2 \rangle}$ of the reaction region. The time dependence of flow build-up has been studied first using the transport model RQMD, for radial flow in [7] and for elliptic asymmetries in [5,8]. From these results it has been inferred that radial flow continues to develop for much longer time than its azimuthal asymmetry.

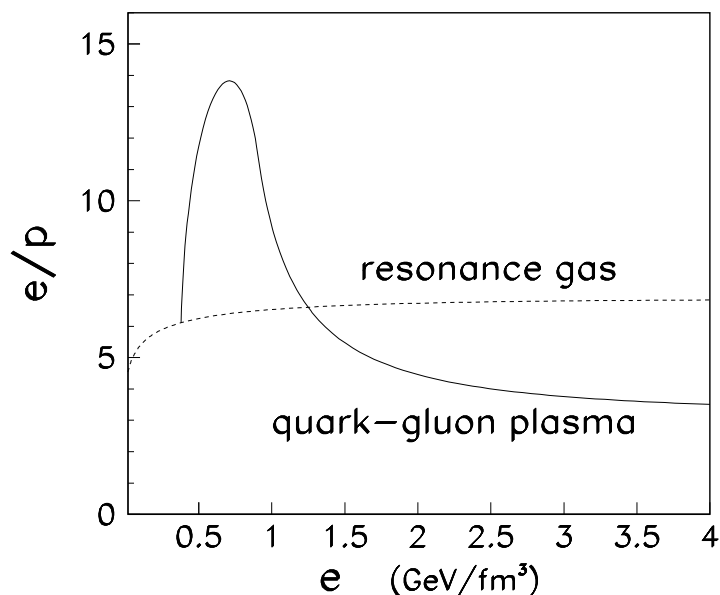


Figure 1. Equation of states implemented into RQMD: ratio of energy density e divided by pressure p . The dashed line represents the resonance gas EOS, the solid line the EOS including a first order phase transition with $T_c=160$ MeV.

One reason for the larger sensitivity of the elliptic flow to early pressure may be that the generated flow asymmetry works against its cause and diminishes the spatial asymmetry on a time scale proportional to $\sqrt{\langle y^2 \rangle} - \sqrt{\langle x^2 \rangle}$. U. Heinz and Collaborators have studied recently elliptic flow in a hydrodynamic framework [9]. Their important finding is that the net effect of the phase transition is much less than naively expected based on this argument. The effect due to the smaller pressure near the phase transition is largely canceled. The system also spends more time in the transition region. On the other side, the *non-ideal* character of the expansion dynamics may be the leading cause that development of elliptic flow is shut off earlier than of radial flow. The elliptic asymmetry is proportional to the difference between the flow strengths in x (parallel to impact parameter) and y direction. Thus it is more fragile than radial flow. Partially free motion (viscosity in hydrodynamic language) tends to wash out the pressure-driven asymmetries.

Obviously, these effects will be more pronounced in the later dilute stages of the reaction. It is amazing that non-ideal effects from viscosity in the low-density stage may be helpful to infer information about the pressure in the high-density region.

Recently, I presented a novel signature of the QCD phase transition based on elliptic flow. A rather characteristic centrality dependence of the elliptic flow – a “kink” – is predicted if the created system passes through the softest region of the EOS in the heavy-ion reactions [8]. Heiselberg and Levy have also put forward arguments why a 1st order transition may be reflected in the b dependence of elliptic flow [10].

Fig. 1 displays the resonance-matter EOS based on counting the propagating quasi-particles in RQMD. In addition, an EOS is shown which is calculated from a quasi-particle model of quarks and gluons with dynamical thermal masses [11]. I have chosen this EOS, because it provides a good fit to lattice data. The EOS contains a 1st order transition at $T_c=160\text{MeV}$ with a latent heat of 467 MeV/fm^3 . In the following results of calculations with the RQMD model which includes either one of these two EOSs will be compared.

Let me first shortly describe how the EOS with phase transition is implemented into the RQMD model[12]. In RQMD nucleus-nucleus collisions are calculated in a Monte Carlo type fashion. While the nucleons from each of the colliding nuclei pass through each other, they are decomposed into constituent quarks. Strings may be excited, and overlapping strings fuse into ropes (with larger chromoelectric field strength). After their decay and fragmentation secondaries emerge and may interact with each other. Formed resonances are treated as unstable quasi-particles. This leads to a resonance gas EOS if there are no corrections from other interactions. The QCD dynamics in the phase transition region is not well understood. Even if there is a quasi-particle description it is not obvious which one of the possible choices (strings, constituent quarks, partons, either massless or with dynamical masses) is to be preferred. In this situation I stick to the implemented degrees of freedom and modify the collision term instead. Since hydrodynamics is expected to be a reasonable approach for the transverse dynamics in the ultradense stage, the EOS should be the most relevant ingredient for the expansion dynamics anyway. It is well-known that different treatment of interactions between quasi-particles may modify the EOS [13]. The standard collision term in RQMD is manufactured such that it does not contribute to the pressure. Now, we let each quasi-particle interact elastically with a neighbor after any of the standard collisions such that the average momentum change leads to a prescribed change of the total pressure according to the EOS $P(e)$. e is the energy density. One should note that this is a reduced EOS, because the temperature T is eliminated. This way we can use the pressure modification not just in equilibrium but also in the non-equilibrium situation. Furthermore, the EOS enters into hydrodynamic equations via $P(e)$. Here we are mainly interested in studying the relation between hydrodynamic flow and EOS.

Let me now turn to a discussion of how a 1st order phase transition affects the centrality dependence of elliptic flow. Experimentally, the elliptic flow can be extracted from the azimuthal asymmetry of final hadrons

$$v_2 = \langle \cos(2\phi) \rangle \quad (1)$$

as a function of centrality. Of course, the spatial asymmetry of the reaction zone which

is correlated with the asymmetry of the participant nucleons in the ingoing nuclei

$$\epsilon = (\langle y^2 \rangle - \langle x^2 \rangle) / (\langle x^2 \rangle + \langle y^2 \rangle) \quad (2)$$

is itself a function of the impact parameter. Trivially, v_2 goes to zero for very small and very large impact parameters because of the corresponding behaviour of the spatial asymmetry. We may disentangle the effects from geometry and dynamics by defining the scaled flow asymmetry as

$$A_2 = v_2 / \epsilon \quad . \quad (3)$$

A_2 represents the dynamical response of the created system to the initial spatial asymmetry.

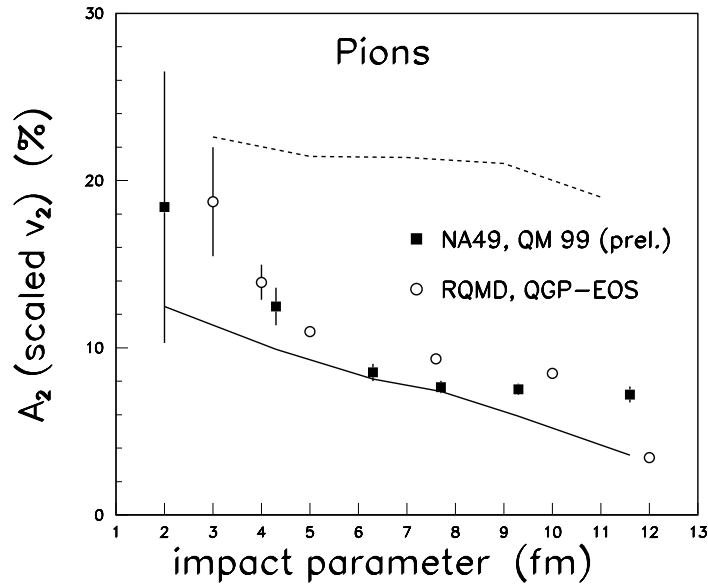


Figure 2. Centrality dependence of scaled flow asymmetry $A_2 = v_2/\epsilon$ for pions in Pb(158A GeV) on Pb: comparison of RQMD results employing resonance gas EOS (solid line) versus EOS with phase transition (open dots), Hydrodynamics (dashed line). Preliminary NA49 data presented by A. Poskanzer at this conference are solid squares.

We display the scaled flow asymmetry v_2/ϵ for pions in the reaction Pb(158A GeV) on Pb versus impact parameter b in Fig. 2. Extracted values from hydrodynamic calculations [6] show essentially no centrality dependence, except for the grazing collisions. All information on the EOS is essentially integrated into one number. For realistic EOSs this number varies between 0.16 [9] and 0.24 [14] for pions. It depends only weakly on other input like the initial or freeze-out temperature. We show a typical hydrodynamical result [14] in Fig. 2. The centrality dependence from hydrodynamics is in marked contrast to the transport calculation which includes the non-equilibrium aspects of the dynamics. Without phase transition, the asymmetry factor v_2/ϵ calculated from RQMD simply increases monotonously with centrality – approximately linearly with the initial system

size in the reaction plane ($\sim 2R_{Pb} - b$). Partial thermalization – so-called pre-equilibrium softness [5] – initially, viscosity and system-size dependent freeze-out play the major role here. Since the resonance-gas EOS $P \sim e$ has no structure, no structure develops in the b dependence of v_2/ϵ either. This monotoneous dependence of the RQMD result without phase transition is also displayed in Fig. 2. (The author is indebted to A. Poskanzer and Collaborators at LBNL who have actually done these RQMD calculations and provided the result.) The RQMD calculation with the EOS with phase transition displays a much more interesting centrality dependence of the scaled flow asymmetry v_2/ϵ . In not too peripheral collisions the system spends more and more time in the “mixed phase” where pressure is low. Thus the increasing initial energy density, reaction time and size which are all helpful to develop the asymmetry are counteracted by the increasing softness of the matter around the softest point of the EOS. A plateau-like b dependence of v_2/ϵ develops (for moderate latent heat values 400–500 MeV/fm³ as in this particular calculation). At some centrality, the softening from mixed phase has developed full strength ($b \approx 5$ fm in the calculation). Further increase of the centrality means that initial high pressure stage from the QGP is added. Therefore v_2/ϵ starts to rise again strongly for $b < 5$ fm. This kink in the centrality dependence of scaled elliptic flow is a combined effect of softest point in the EOS and non-equilibrium due to smallness of the system. Neither one of these effects separately produces the characteristic “kinky” b dependence. This has been demonstrated by the hydrodynamical with phase transition and the RQMD calculation without. It should also be noted that the RQMD calculation approaches closely the hydrodynamical result for very central collisions. This is precisely what one would naively expect about the system size dependence of non-equilibrium corrections.

At this conference, A. Poskanzer has presented first experimental data for the scaled flow asymmetry v_2/ϵ (see his contribution to these Proceedings). The RQMD prediction was taken from [8] and has narrower rapidity coverage than the preliminary NA49 data. A quantitative comparison has therefore to be taken with some grain of salt. Nevertheless, it appears that the experimental measurement follows more closely the trend of the result if the phase transition is included in the EOS. Of course, in view of this exciting indication one would like to see data (and model results) with much better statistics in the region $b < 5$ fm. p_t cuts (low p_t pions show an even more pronounced kink) and consideration of other hadrons (protons, strange particles) would also be of interest.

Study of the b dependence may also be very useful for Au on Au collisions at RHIC. What to expect depends essentially on the (so far unknown) particle densities. If the RQMD approach continues to describe hadron multiplicities well, such a kink can also be expected. If initial particle production is much higher than RQMD predictions, the hydrodynamic result may be closer to reality. Elliptic flow in collisions at RHIC has been studied using a parton cascade model [15]. Some dependence on viscosity (magnitude of cross section) has been found, but the hydrodynamic result (for the hard EOS without phase transition $P = e/3$) has been qualitatively confirmed. Elliptic flow holds much promise to clarify the structure of the EOS in the phase transition region – at finite temperature and also baryon density (see P. Danielewicz’s talk).

2. Quo Vadis, Heavy-Ion Theory?

Recently, B. Mueller has provided the write-up of his summary talk given at QM '99 [16]. He expresses his opinion what should be done in heavy-ion theory and – more explicitly – what should not be done. (According to him the study with RQMD whose results have been presented in the first section should **not** have been done.) Taking his contribution at its most positive aspect, it expresses a need to reflect on the direction of heavy-ion physics. I feel this need as well, not just in view of RHIC coming up soon but also due to the uncertainty about the merits and prospects of heavy-ion physics. On the other side, I strongly disagree with the direction for heavy-ion theory which Mueller advocates. The problem is not mainly with this particular direction but its “absoluteness”, i.e. that it is just one direction out of several possibilities. Expression of beliefs and dogmas are no substitute for proofs or disprovals in science. Mueller’s feverish rhetorics (“nonsense”) or threats (“do not even think of applying these models at RHIC”) would be expected in preachings of a sect but not in a summary talk at a physics conference [17].

There are three types of approaches in theoretical heavy-ion physics, the thermal fireball, hydrodynamics and transport theory. These different approaches complement each other and will continue to persist side-by-side. Their mutual usefulness is not just an abstract statement but has been proven in the past and at this conference as well. Indeed, a five-minute analysis of transverse hadron spectra in the framework of the static thermal fireball model gives reasonable values for kinetic freeze-out temperature (100–140 MeV) and flow velocity ($v = 0.4 - 0.6c$) for Pb(158A GeV) on Pb collisions. Furthermore, particle ratios tell that chemistry is different and points to a clearly higher freeze-out “temperature”. Fine. (It has been predicted by RQMD at a time when Stachel, Braun-Munzinger and Collaborators were arguing for a common kinetic and chemical freeze-out based on fireball results for single-particle spectra [18].) Do we need more sophistication than provided by the fireball model? Yes. If not for anything else, different independent approaches may be used to uncover elementary mistakes in too simplistic models. Mueller’s talk provides a good example how reliance on the most simple one – the static fireball – produces disastrous errors. Mueller applauds Wiedemann to have presented (at this conference) “a highly consistent picture (as) emerging from these measurements. The single particle momentum spectra, the p_t -dependence of the pair correlations, and the fragment yields all can be explained by a freeze-out from a thermal, dilute hadronic fireball with a final temperature (at the SPS) around $T_f \approx 120$ MeV, an average transverse flow velocity $\langle \beta_f \rangle \approx 0.55$, and a baryon chemical potential $\mu_f \approx 250$ MeV. The rms radius of the fireball at freeze-out is slightly above 10 fm, and the average freeze-out time is about 6 fm/c spread over a rather short period of 3 fm/c”. These numbers refer to the system Pb(158A GeV) on Pb.

Now, $s = s_0 + \beta \cdot t$ (constant velocity) or $s = s_0 + \frac{1}{2}\beta \cdot t$ (constant acceleration) are indeed formulas which somebody talking about velocities should be aware of. Let us take the most extreme case in which all flow velocity is created instantaneously (quite unrealistic), the first formula. How can a system of initial rms size 4 fm (given by the wounded nucleon participants of the colliding two Pb nuclei) expand to 10 fm within just 6 fm/c – but with half the velocity of light? A more realistic acceleration history, e.g. as given by the second formula, magnifies the problem. R. Stock presents somewhat different

numbers, based on the same fireball approach and for the same system [19]. However, his – slightly higher – value for the “life-time” (8 fm/c) but same value for transverse expansion as Wiedemann’s (factor 2.5) appears also incompatible with any dynamical expansion model, be it hydrodynamic or transport approach.

This example may clarify to which erroneous conclusions a self-imposed restriction to the simplistic fireball model which has not much to say about time evolution (dynamics) may lead to. A look at this author’s prediction for final flow velocities, (average) freeze-out “temperatures”, source sizes and life times based on transport calculation with RQMD [20] might have been helpful to avoid such hazard. I should add for non-initiated readers that the value of these numbers for life-time etc. is of tremendous importance for a qualitative understanding of the expansion dynamics. Is it an “explosive expansion pattern” as Stock put it (a bang) or soft transverse expansion which I among others [21,22] have argued for, a “fizzle” (a word which I borrow from E. Shuryak)?

The content of the debate with Mueller is which strategy may be successful to understand and approximate quantum chromodynamics (QCD) of dense matter as it is produced in ultrarelativistic heavy-ion reactions. If we forget non-equilibrium features, it is the physics of QCD at $1\text{--}2\ T_c$, with the “critical temperature” around 150–170 MeV. Since equilibrium situation is much simpler, I start with some remarks about finite T QCD. Smilga among others has pointed out that the physics of the state above and close to T_c is a theoretical “no man’s land” [23]. I share this opinion. Neither perturbative QCD nor interactions between (quasi-)hadronic states appear justified for a description. At hadron densities of one per cubic fermi or equivalent quark densities even the pragmatic question from which single-particle base to start (hadrons or quarks) is not easily answered. We have to do the physics at RHIC and LHC facing the fact that there is a wide hole in our knowledge of QCD in the most interesting temperature region.

The gap in our knowledge cannot be filled by statements of belief like the one from Mueller’s talk: “the picture of QGP as a plasma of weakly interacting quasi-particles ... may work until very close to T_c ”. Maybe, but maybe not. I do not want to be mistaken. The quasi-particle approach is a very important strategy to resolve some of the problems which have plagued perturbative calculations for thermal QCD so far. In fact, to my knowledge I have been the first person to utilize the EOS as calculated within such a quasi-particle approach for (effective) quarks and gluons in calculations of heavy-ion reactions. On the other side, nobody knows yet how good the quasi-particle picture is, in particular in the range $1\text{--}2\ T_c$. Agreement on the EOS level does not tell much, because many rather different interacting systems may have the same EOS. A “screening mass” is a much better measure of correlations in the plasma than the EOS. Kajantie et al. have erected a strong warning sign in that respect. This group has shown that the perturbatively calculated screening mass approaches the real one only at astronomically large temperatures (at which QCD itself is invalid) [24]. Roughly, in the temperature range of interest the real and the perturbative screening mass differ by a factor of 3–4. That means that the perturbatively calculated viscosity (among other transport coefficients) can be expected to be wrong by the square of that number, i.e. an order of magnitude.

Of course, non-equilibrium situation in heavy-ion reactions is much more complicated than the finite-temperature case. Therefore all what was said before about the lack of knowledge applies even more in this situation. Furthermore, for heavy-ion reactions we

would need to know how a system of quarks and gluons hadronizes. Since that is connected to the mystery of (de-)confinement a resolution belongs yet to the realm of Utopia. Either one gives up in such a situation or one resorts to *modeling* which is the typical strategy in physics. As usual, there are flawed, bad and better models, and we will have to say more about this specifically later on. Moreover, since no model can be completely trusted, it is very important to test a broad variety of models as much as possible against the “true” theory and available data to sort out generic features from arbitrary model details.

In contrast, Mueller argues against use of what he calls “hadronic models” to describe collisions among heavy nuclei at the SPS. According to him they are (partially) pure fantasy, consequence of the imagination of its author. Perhaps. Every model carries this risk. Unfortunately, Mueller did not reflect on that this might apply to his own model creation (together with K. Geiger), the parton cascade model as well. (The lattice data for the EOS are in clear conflict with a weakly coupled gas of almost massless partons.) Actually, I know only of few hadronic models applied at SPS energies, Kahana²’s Lucifer, Kapusta’s and Jeon’s Lexus and Cassing’s HSD. Mueller subsumes “string” models like RQMD and VENUS into this category which I believe is a misspecification. In these models in-going hadrons are destroyed into multiple components on the valence and sea quark level. Of course, they behave differently (create strings) than perturbatively interacting partons. However, in view of the theoretically unsettled situation it is not obvious a priori whether this should count positively or negatively.

What are the pro’s and con’s of the various types of microscopic dynamical models? In order to discuss the range of applicability one should take into account that nuclear reactions are characterized by vastly different stages, schematically

- **the first fm/c:** destruction of the ingoing hadrons (nuclei) and multi-particle production,
- **the hot, ultra-dense stage** (which includes the possible phase transition), and
- **the dilute hadronic stage** until freeze-out. (The proper treatment of this stage is not contentious.)

Main contenders for the “initial-stage” physics are “soft production” models based on Regge theory supplemented with the string picture and “semi-hard production” models (Mini-jets as studied by X.N. Wang, M. Gyulassy and K. Eskola, Geiger’s and Mueller’s parton cascade and at ultrahigh energies the semi-classical Weizsäcker-Williams type approach advocated by L. McLerran, R. Venugopalan and Collaborators). To some degree, the two pictures are complementary – with strengths and weaknesses in different areas.

Perturbative QCD based approaches: the natural starting point is to look at high p_t particles, understand their properties and then march towards lower p_t . At which p_t – perhaps dependent on hadron species – does the model break down and “soft physics” (flow, etc.) sets in? Recently, X.N. Wang has studied high p_t pions at SPS energies (see his talk). Contrary to expectations about strong energy loss of partons in the dense medium his finding is that the partons seem to experience essentially no loss at all. That is one of the most important results in HI physics of the past years. Geiger’s and Mueller’s parton cascade model (VNI) overshoots the WA93 data by a factor of 5–10 at high p_t [25] where the model should work. Why not? Maybe Mueller, Srivastava and

Collaborators could look for answers. My suggestion would be that the problem is in the space-time ordering of partons with different rapidity. A further hint of problems with present parton cascade VNI has been provided by Longacre recently [26]. Energy and momentum is created outside the future light cone of the two colliding nuclei. It appears to be a strong violation of causality. Recently, VNI has been tested by the OPAL group for $W^+W^- \rightarrow q\bar{q}'q\bar{q}'/l\bar{\nu}_l$, a system of few hard partons [27]. In contrast to all other tested pQCD based codes (PYTHIA, HERWIG, ARIADNE) it failed badly in reproducing the event properties. Structure functions are distributions in momentum space, but a parton cascade needs initial condition in phase space (space-time and momentum). We urgently need more studies how to initialize a parton cascade in phase space.

Regge theory, strings, baryon junctions, ropes and all that: cumbersome things of the past and irrelevant for RHIC physics? The “excommunication” of such non-perturbative phenomena from HI physics is suggested in B. Mueller’s summary talk. I beg to disagree. There were always strong indications that QCD at large distances may be expressed as a theory of interacting strings. t’Hooft made the argument based on large N_c in the 70s. Of course, the real reason predates QCD. It was Regge theory which was abstracted “empirically” from strong interactions in the soft domain. Everybody knows since Veneziano’s work in ’69 that string theory is the natural candidate underlying Regge physics. It was one of the most important developments initiated by Witten [28] and others in the course of last year (and has gone unnoticed at QM ’99) that the idea of *duality* between gauge theory in the large N_c limit and string theory may be put on firm grounds by extending Maldacena’s conjecture into the non-supersymmetric domain.

There are many aspects of the string-gauge theory duality which may be fruitful to heavy-ion physics. One of them goes to the heart of the nature of the QCD phase transition: if gauge theory can be mapped to a certain string theory, then string theories at finite T should not contain only the possibility of a limiting (Hagedorn) temperature but also of a phase transition. It is noteworthy that the “empirical” QCD transition temperature extracted from the lattice (≈ 160 MeV) coincides with the Hagedorn temperature estimated from the spectrum of hadronic resonances.

Another important issue for RHIC physics is baryon stopping. In a string model like RQMD baryon shift is a deeply non-perturbative process, the stripping of valence quarks off the “baryon junction” (which is connected to the valence quarks via strings). D.J. Gross, one of the fathers of QCD, and Ooguri co-wrote a paper in which the large N_c baryon wave function is constructed from N_c strings connected via a junction using some super-gravity solution (and employing the recently conjectured duality to gauge and string theory) [29]. A highly non-trivial prediction of the junction dynamics in RQMD is that baryon number is stopped more than the valence quarks. Why not look for it experimentally in order to see whether it is a fictitious component of the model?

Color ropes are flux tubes of chromoelectric field created by charges higher than the elementary SU_N color charges. In RQMD they form if strings would overlap. (The idea of coherent superposition of gluon fields from valence quarks of different nucleons has also been the starting point of McLerran and Collaborator’s work, however, in the perturbative domain.) In contrast to Mueller’s claims, ropes have been studied from first principles, in lattice gauge theories [30]. Important properties needed for phenomenology like the transverse size independence on the color field strength have been confirmed on the lattice

[31]. Whether they decay via Schwinger-type particle creation like it is assumed in RQMD I do not know. It may be tested experimentally, however. Coherent fields are *stronger* than incoherent ones. Therefore they are screened earlier. This effect is at the root of the result why particle production at RHIC is so low in RQMD. A charged hadron dN/dy of approximately 700 is predicted for central Au on Au collisions. Incoherent multiple-scattering models (independent whether they follow parton cascade recipes or Regge theory) predict a much higher rapidity density, 1500–2000. In a couple of months from now we will know the real answer and can review the existing theoretical attempts in light of the experimental findings.

In his summary, Mueller provides one “dramatic revelation” why strings should not be considered at SPS energies. It is a recent result by Bass et al. [32] according to which most of the energy density up to a time of 8 fm/c (in Pb+Pb) resides in sterile (=non-interacting) strings. I find this result rather cumbersome, and it is not found in a string model like RQMD. Generally, time scale of string fragmentation is governed by string tension and quark masses which leads to the famous formation time of around 1 fm/c. Such formation time has been observed in p+A experiments. Which physics leads to a scale of 8 fm/c for string decay? (A possibility would be that a Lorentz factor γ has sneaked somewhere into the calculation.)

Finally, I would like to add one comment concerning the modeling of the the hot, ultra-dense stage of HI collisions including the phase transition. Certainly one should explore the transport theory of quark and gluon quasi-particles in the plasma and for non-equilibrium situations. Hopefully these concepts will work at temperatures not too far from T_c , say $2 T_c$. There are plenty of observables in HI collisions at RHIC (and LHC) for which predictions are needed, photons and intermediate-mass dileptons, jet quenching etc. – with sensitivity to high-temperature physics. Nevertheless, physics of the phase transition will be quite a different situation. A factor of 2 on the temperature scale corresponds to a factor 16 or so on the energy density scale. There are two related – practical – reasons why use of hadronic quasi-particles is sensible, above T_c and keeping all the caveats about unknown properties of these objects in mind. They come into play why I do not share Mueller’s belief that coupling of parton cascade with hadronic “afterburner” – like being done by Bass et al. [33] – is the only reasonable strategy for transport calculations at SPS and at RHIC. One of the most important reasons to do transport is to study the EOS in the phase transition region including non-equilibrium effects. Seen from the quark-gluon side, this is mainly the physics of the (effective) bag constant, the difference between perturbative and real vacuum. Without that there is no physics of the softest point. If we estimate the difference between the two vacuum energy densities as on the order of 400 MeV/fm³ we better stop calculations in the “wrong” vacuum at energy densities which are large against this value (or we take the difference into account). The second reason is that transport theory loses its advantages to hydrodynamics if detailed balance is given up. Detailed balance follows from time reversal invariance. Without it, a system out of equilibrium is no longer driven towards equilibrium. Some arbitrary “one-way” prescription for a “parton–hadron” transition is a massive violation of detailed balance. I believe that many successes (and failures) of transport models have to do with (no) implementation of both directions in transition processes. They tend to make the chemistry of the reaction insensitive to the microscopic transition rates.

(Un-)fortunately, QCD is a complicated theory. It should be treated like that.

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